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TRENDS OF MODERN BIOLOGY[1]

I

AN occasion such as this is thought-provoking. Why should anybody endow a chair of biology? When I began the study of the subject a little more than a quarter of a century ago such things were not done. In most of our large universities biology had a fairly secure position, but in all but a very few of the small colleges, at one of which I am proud to say I had the privilege to study, if present at all it was so distinctly only on sufferance. Much doubt existed and was often expressed as to whether this novel subject had any disciplinary value in the training of the youthful mind, or had any particular cultural worth in the producing of better citizens. Those of us who were irresistibly lured, by the fascination of the wonderful field opened to our vision, to spend most of our time in the biological laboratory, were looked upon by our fellow collegians as queer freaks of nature, and would certainly have been called Bolsheviks had that overworked appellation been current verbal coin in those days. For the subject distinctly lacked respectability. It was thought by those who pursued the classics or other orthodox lines of educational conduct to be a messy business, was known to be smelly, and was generally held to be low. This attitude inevitably called forth a defense reaction on the part of its callow devotees, which resulted in distinctly worse messes and smells than were really requisite for the successful pursuit of knowledge in the field.

Now all this has changed. Biology has come

[1] Papers from the Department of Biometry and Vital Statistics, School of Hygiene and Public Health, Johns Hopkins University. No. 80.

An address delivered at Mount Union College, Alliance, Ohio, October 20, 1922, on the occasion of the dedication of the Milton J. Lichty Chair of Biology in that college.

into its own, and the security of its position in the educational world can not be shaken even by so doughty a champion of the powers of intellectual darkness as Mr. Bryan. What has happened in these twenty-five years in biology? And what of the present and of the future? Can we find in the efforts and achievements in this field due warrant for that intellectual respectability that biology has now gained, and for that clear faith in the future which is implied in Dr. John A. Lichty's splendid endowment which we are here gathered to dedicate?

Perhaps as good a method as any of getting light on this matter will be to attempt a review of the major trends of biology in the past and the present. In doing this we shall find that in every case these trends of thought and research have been responses to some quite naïve and simple bit of intellectual curiosity, of the sort likely to arise in a child's mind, if he turned his thought at all to living nature about him. It may fairly be said that up to the time of Darwin and Wallace and the "Origin of the Species," all biology busied itself with the answering of one phase or another of the following two naïve questions:

First, how many and what different kinds of animals and plants exist, or have existed, on the face of the earth.

Second, regarding living animals and plants as ingenious and complex contrivances, but after all not fundamentally unlike other contrivances, how are they put together and how do they work?

Every boy and girl who collects butterflies, or who pulls a wasp to pieces in order to locate and with safety observe the behavior of its "stinger," is in a rough and ready way repeating in his own development the history of the growth of our present knowledge of biology. He is trying on the one hand to get together a collection of the different kinds of living things about him, and on the other hand to inform himself as to their structures and functions.

Since the publication of the "Origin of Species" a third question, essentially just as naïve, but less easy to deal with objectively and practically, has occupied a great part of the attention and effort of biologists. But that it indicates a sort of intellectual curiosity not essentially one bit more sophisticated than the other two, is plain enough if we remember that all peoples to the remotest historical time, and including even savages, have not only thought about it, but also have had theories about it. This question we may put in this way:

Third, whence, why, and how came the animals and plants which inhabit the earth to be here at all?

It is, as I have said, in an attempt to answer these three questions, in some one or other of their aspects, that all we know to-day about biology has developed and grown. It is an impressive fact, recently discussed with great brilliancy by James Harvey Robinson[2] that always in science, biology no less than all the rest, the motivating problems which have led to the advancement of knowledge have been simple naïve questions about quite commonplace things. He says:

> Those to whom a commonplace appears to be most extraordinary are very rare, but they are very precious, since they and they alone have made our minds. It is they who have through hundreds of thousands of years gradually enriched human thought and widened the gap that separates man from his animal congeners. Without them the mind as we know it would never have come into existence. They are the creators of human intelligence. The mass of mankind must perforce wait for some specially wide-eyed individual to point out to them what they have hitherto accepted as a matter of routine or failed altogether to notice. These mind-makers are the questioners and seers. We classify them roughly as poets, religious leaders, moralists, story-tellers, philosophers, theologians, artists, scientists, inventors. They all are discoverers and pointers-out. What eludes the attention of others catches theirs. They form the noble band of wonderers. Commonly unnoticed things excite a strange and compelling curiosity in them, and each new question sets them on a new quest. They see where others are blind, they hear where others are deaf. They point out profundities, complexities, involutions, analogies, differences and dependencies where everything had seemed as plain as a pike staff.

Robinson, in what I have quoted, lays em-

[2] Robinson, J. H.: "The Humanizing of Knowledge," SCIENCE, N. S., Vol. 56, pp. 89-100, 1922.

phasis on the kind of man who sees the problem. Perhaps it may help by ever so little in the production of such men in this laboratory which we are starting on an enlarged career of usefulness to-day, to emphasize the importance for success in biology of being simple-minded.

II

Our first question about the different kinds of living things which people this earth led to the important branch of biology which is called taxonomy or classification. This was for a long time the dominant trend of the subject. The first step toward a proper knowledge of the phenomenal world is obviously to get the phenomena classified in an orderly scheme. In biology this takes the practical form of getting different kinds of plants and animals described, named and classified. Linnæus was able to classify all the plants and animals known up to 1735. Nowadays no one person would think of attempting so colossal a task, and if he did would fail by virtue of the inadequacy of the human life span. Instead we find the worker in the branch of biology to-day devoting his life to one, or at most a few, groups of animals.

From its once dominant position taxonomy has apparently fallen to-day, one must reluctantly confess, into rather lower repute in the mind of the general biological public. Neither our professors nor our students of biology appear, with a few brilliant exceptions, to be interested in it. One forms the impression that perhaps four fifths of the Ph.D.'s turned out in zoology at the present time not only never have, but probably never will, for themselves, identify an animal strange to them, and as for deciding whether the unknown creature has been previously described, or placing it in proper taxonomic relation to its nearest relatives, such a problem would be as far beyond their powers as it is beyond their desires. By a curious paradox many modern biologists take precisely that attitude towards and about the living world around them in the practical conduct of their every day working life, which they would logically be expected to take if it were their deepest conviction that each living thing were the product of an act of special creation—God-given and therefore not to be worried about—and that such a process as evolution had never occurred.

Yet it is beyond question that if a young man embarking on a biological career has a desire to make an enduring contribution to knowledge, of permanent value, and incapable of being upset by any future developments of the subject, his best chance of doing this laudable thing is by becoming a careful, accurate taxonomist. If he describes accurately, carefully and completely a hitherto undescribed species of animal or plant, in such a way that any one who reads carefully the description can recognize and identify the thing described, he has chiseled for himself an indelible record in the history of man's intellectual progress.

Some there are who will argue that while what has just been said may be true, the niche in the tablets of history carved in this way is too slight to be of any significance, that, in short, systematic or taxonomic work has only a small and unimportant intellectual content, as compared with other sorts of biological study. Such a view of the case seems to me to be singularly lacking in vision. It means that the commonplace elements in taxonomic work have been allowed to overwhelm in their view its broad and deep significance. The labors of the taxonomists have alone given us such picture as we have of the inter-relationships, unity in diversity, and diversity in unity, of animate nature as a whole. It is the systematist who has furnished the bricks with which the whole structure of biological knowledge has been reared. Without his labors the fact of organic evolution could scarcely have been perceived, and it is he who to-day really sets the basic problems for the geneticist and the student of experimental evolution. His facts are the raw material from which the laws of organic evolution, in the sense that we speak of physical laws, must be worked out. An example of what is apparently a real law of organic evolution, deduced directly from the simplest taxonomic statistics, is found in the fact that the sizes of genera of plants and animals, as measured by the number of species each contains, are not distributed in frequency accord-

ing to the normal curve of error, as most chance determined phenomena are, but instead obey with extraordinary exactness, as has been shown by Willis and Yule,[3] the rule that the logarithms of the *frequency* of genera plotted to the logarithms of the *size* of the same genera (*i. e.*, the number of species in each), give a straight line.

It is with much satisfaction that we find the leading exponent of the reigning mode in present-day biology, Bateson,[4] saying of taxonomy:

I had expected that genetics would provide at once common ground for the systematist and the laboratory worker. This hope has been disappointed. Each still keeps apart. Systematic literature grows precisely as if the genetical discoveries had never been made and the geneticists more and more withdraw each into his special "claim"—a most lamentable result. Both are to blame. If we can not persuade the systematists to come to us, at least we can go to them. They too have built up a vast edifice of knowledge which they are willing to share with us, and which we greatly need. They too have never lost that longing for the truth about evolution which to men of my date is the salt of biology, and the impulse which made us biologists. It is from them that the raw materials for our researches are to be drawn, which alone can give catholicity and breadth to our studies. We and the systematists have to devise a common language.

The separation between the laboratory men and the systematists already imperils the work. I might almost say the sanity, of both. The systematists will feel the ground fall from beneath their feet, when they learn and realize what genetics has accomplished, and we close students of specially chosen examples may find our eyes dazzled and blinded when we look up from our work-tables to contemplate the brilliant vision of the natural world in its boundless complexity.

It seems probable that we shall before long witness a return to a saner attitude than has prevailed in the last quarter of a century in regard to systematic zoology and botany; and in the training of our students, by not beginning specialization too soon and too violently, give them a more adequate conception than they now get of the orderliness and the diversity which together characterize animate nature as a whole.

III

The dominant mode in biology in my student days was morphology. I was nurtured on the somewhat arid problems of vertebrate cephalogenesis and the components of the cranial nerves. Probably few students in these days are excited by such problems. A vague awareness that there are such things as cranial nerves no doubt suffices and everyone is just as happy. The whole subject of pure morphology, as it was cultivated twenty-five years ago, seems singularly sterile now. It was a highly developed discipline, with a set of rules as rigid, and also be it said about as soul-stirring, as those of the Greek grammar. In its fine spun theories about homology, metamerism and the like, biology got off on a wrong track, which, as is now practically universally admitted, had only a blind ending.

But this does not mean, as those of the younger generation are apt rashly to conclude, that the old morphology was of no value. Intrinsically it was of great value. Few things will transcend in importance in the study of biology, the finding out of all that can be learned about the way in which living machines are put together. As long as this purely descriptive purpose was the primary and essential object of morphological study, all was well. The business only began to go bankrupt when it took on an essentially metaphysical purpose, and a logically bad, not to say hopeless one, at that. For what the pure morphologists of the eighties and early nineties were trying to do was to infer from purely static phenomena (the intimate structure of the body) the dynamic relations in a course of events (organic evolution). Such a task would have been perceived to be hopeless long before it was, except for the seductive lure of certain rules by which the game was played, which rules (such as ontogenetic recapitulation

3 Willis, J. C., and Yule, G. U.: "Some Statistics of Evolution and Geographical Distribution in Plants and Animals, and Their Significance," *Nature*, February 9, 1922, pp. 177-179.

4 Bateson, W.: "Evolutionary Faith and Modern Doubts," SCIENCE, N. S., Vol. 55, pp. 55-61, 1922.

of phylogeny, certain aspects of homology, etc.) were mistakenly supposed to be natural laws, whereas in point of fact, at the best they were only imperfect expressions of certain inherent necessities of the philosophic principle of organization, and at the worst just plain buncombe.

It is unfortunate that in the reaction against this sort of thing which has occurred in the last quarter-century the pendulum has swung so far as to deprive the present day student of biology of a good deal of the exact rigid morphological training that he got in earlier days. There never has been any better training for hand and eye and mind than that which went with the getting of an adequate understanding of the comparative anatomy of the vertebrates, no matter what field of biology the student subsequently entered upon as a specialty. So generally inadequate is the training in this field, now, I am told, that several of our best medical schools have found it necessary to devote a not inconsiderable part of the time allotted to anatomy in the medical curriculum, to the study of vertebrate comparative anatomy, because it is essential to the right understanding of human anatomy, and the students do not have it when they come, although they have the bachelor's degree and have been required to take biology.

We have seen, in the brief sketch which has so far been given of the course of biological events, that two trends of thought and research that were formerly of major importance have on the whole fallen somewhat into a state of desuetude. It will pay us to inquire a little more carefully into the reasons for this change of interest and esteem, because otherwise we are apt to reach the erroneous conclusion that taxonomy and morphology were never of any real importance or significance in the development of human knowledge, and that our forefathers only deluded themselves in thinking that they were. The fundamental reason for the decline in the cultivation of these two disciplines has already been touched upon. It is found in the fact that taxonomy and morphology, as originally practised in their pristine purity, dealt solely with static aspects of vital phenomena. Now the only thing of really compelling interest and significance about life is its dynamic character. Organisms live and do things. It is only this which makes them more interesting than bricks or paving stones. But by a curious quirk of the evolution of intellectual matters, the only group of people, before the publication of the "Origin of Species," who, as a group if they perceived this somewhat obvious fact, did anything about it, were the physiologists.

The historical development of physiology was bound up with and a part of that of medicine, rather than what we now call general biology. The first systematic treatise professedly dealing with physiology as an integral part of general biology was Claude Bernard's "Physiologie générale" and appeared only in 1872. The significance of this is that, in the main, and with only a few notable exceptions, those who prior to that time had been interested in physiology had been almost wholly concerned with workings of the mechanisms solely of the human body, and even in this somewhat narrow field, the significance of the findings for the science and art of medicine held the foremost place in esteem. All this has, of course, changed with the considerable development during the last quarter of a century, of general physiology under the leadership of such men as Loeb in this country, Bayliss in England, and Verworn in Germany.

But at its best physiology concerns itself chiefly with only certain of the *internal* dynamic phenomena of living things, and this is only a small part of the sum total of the activities which constitute life. That all biology should primarily be concerned with dynamic matters was first brought powerfully to the attention of thinking men by Darwin. The significance of Charles Darwin's work upon the intellectual development of his and subsequent times has been variously described and estimated. If we go down to real fundamentals it seems to me that we must conclude that one of the most important elements, at least, lies in the making it so plain as never again to be misunderstood, that the essential problems of biology are questions of dynamic relationships and not of static phenomena.

The immediate effect of Darwin's work, at

least so far as zoology was concerned, was a curious one. It led to an enormous development of research in what is perhaps the most essentially static branch of biology, namely, pure morphology. The process of reasoning was something like this. Since evolution leaves a record of its progress in the structures of animals, by studying these structures intensively it ought to be possible to reconstruct not only the course, but even also the method, of evolution. Von Baer's so-called law, to the effect that ontogeny repeats phylogeny, was held to be the key that would unlock all the secret places of organic evolution, and the biological world went more or less mad over embryology.

But as has already been pointed out, this line of attack proved to be sterile, so far as the problem of evolution is concerned. Ontogeny does not repeat phylogeny with anything like that degree of fidelity which would be required if it were to be the means of unravelling the tangled thread of evolutionary progress. And the observed static end results given by the structures of existing animals are capable of being produced in too many different ways, as we now know, to make possible any precise conclusions from the mere study of their form as to the dynamic course of events which led to their existence.

IV

When this fact had become evident and sunk deeply into the consciousness of the working biologists, the way was cleared for the beginning of the great movement towards modern general biology. It is an odd mischance of fate that Darwin, who is the real founder of modern general biology, should not have seen any of its fruits in the declining years of his life, but instead only an abortive development resting on a ridiculously unsound philosophy. When biology, at the very end of the nineteenth century, got once more on the right track (for much earlier in its history it had been there, and only got diverted by a bad philosophy as to how the problems of evolution could be solved) a new world was indeed opened to our vision. And the password to it was experimentation. To the working biologist organisms once more became living things, not desiccated or pickled corpses. I cannot recall that in my undergraduate days there ever was a *living* animal in the laboratory, with the exception of protozoa. Certainly none was ever studied in any but a thoroughly pickled condition. As one looks back now on those days he is horrified not alone at the tortuosity of the intellectual pathway by which we attempted to come upon a knowledge of life, but also at the awful waste of alcohol!

The keynote of the new biology was dynamic and its methods were, in the main, experimental. Each of the old disciplines took on a new life. Morphology became experimental morphology; evolution became experimental evolution; a new shoot, ecology, sprang up from the gnarled old root of the taxonomic tree; and in some sense as the crowning glory of the whole edifice, animal behavior and comparative psychology began to flourish and attain a respectability never enjoyed by the labors of the old-fashioned naturalist, who observed what he called the "habits" of animals and plants.

Since these movements I have named comprise nearly the whole of the major trends of biology in the twentieth century it will perhaps be worth our while to examine a little more carefully into the philosophy and significance of each of them. For on and out of them is to grow the biology of the future, with all the great advances in knowledge which it has in store.

V

Modern experimental morphology may fairly be said to begin with Roux. His philosophy may be summarized in this way: organisms are machines which in their operations follow the laws of mechanics. Their structures are as they are because of the operation of these laws upon the plastic and adaptable material of which they are composed. It is the task of developmental mechanics to discover the specific physical and chemical laws which determine the form of particular structures of the living body. On the whole the most feasible way to go about accomplishing this result is to observe the results which follow upon the experimental modification of the physical and chemical conditions which environ the embryonic de-

velopment of particular structures. Then in the favorable case we shall be able definitely to connect and correlate particular physico-chemical events with particular biological events in a causal way. We shall replace metaphysical speculation in the field of morphology with observed physical causation.

The results of the last quarter century have abundantly justified the faith of Roux and his followers in soundness of this philosophy. So close are we to the events themselves, however, that we cannot justly appreciate, I believe, the enormous significance of the advance in our knowledge of the fundamentals of biology which have come as the result of the labors in this field of a host of workers, under the leadership of Roux in Germany and of Morgan in this country. The important advances in this field have, in the main, come from these two countries.

The great activity in the fields of experimental morphology and developmental mechanics has also been in considerable degree responsible for the growth and healthy condition of another major trend in modern biology, namely cytology. This is pure morphology at its best, resting on the sound philosophical purpose of the exact description of the minute anatomy of the cell. In this field America has again been a leader. E. B. Wilson's book,‘ ‘The Cell in Development and Inheritance," may well be said to mark an epoch, at least in American biology. The achievements of cytology in the last quarter century have been of no mean importance. This field of research, for example, has played the leading role in clearing up the age old problem of the determination of sex. The discovery by McClung of a mechanism in the germ cells, the accessory or sex chromosomes, and the subsequent great extension and solid grounding of this knowledge by Wilson and his students, have served to take out of the realm of mysticism and put into the clear light of ascertained fact the answer to one of the great biological riddles. Again, in this same period cytological research has laid the structural foundation of the mechanism of heredity. The student of the history of science will note here an interesting fact. Discoveries of major importance in regard to dynamic biological events have here been made by a purely static, descriptive mode of research. This is unusual. Why it has happened so fortunately is because the American workers in cytology, in the period of which we are speaking, have at every stage worked in the closest touch with the experimentalists, and have directed their descriptive studies to problems which have made themselves compellingly obvious from and in the experimental work which was going on at the same time, and in many cases in the same laboratory. A static method has worked in correlation and cooperation with a dynamic experimental method. We see beautifully exemplified here one of the main functions of descriptive science in general, in relation to experimental science. The descriptive worker endeavors to lay the structural foundation of the dynamic events with which the experimentalist directly concerns himself. The fruitfulness of this method and ideal of work in morphology, as compared with sad sterility of the point of view which vainly attempts to solve *in toto* dynamic problems by a purely static mode of research as the older morphology did, is apparent in the recent history of biology.

VI

Jennings has somewhere said that "An animal is something that happens." While this happy phrase might well be taken as the slogan for all modern biology, it expresses with particular aptness the point of view of that major trend in recent biological history in which its author was the one of the most considerable pioneers and leaders, namely the study of animal behavior. The development of this subject into the prominence it has enjoyed in the last quarter of a century does not represent altogether quite so sharp a break with the philosophy of an earlier time as was the case in the development of experimental morphology. The field naturalist had always properly esteemed the importance of things which happened, and there exists, in the older literature of popular and amateur natural history, a considerable mine of rather accurate observations about the behavior and habits of

animals under natural conditions. Perhaps some day students of animal behavior from the modern view-point will adequately work this body of ore. It will not be an easy, nor a completely profitable task. The trouble of course is that, generally speaking, the naturalist of the old school was not analytical, but rather anecdotal, in his interest in the behavior and habits of animals.

It was just this difference that marked off the new school of animal behavior from the old. If what living things do is the most important consideration in distinguishing them from non-living things, it would seem clear that our knowledge of biology in general is bound to be increased if we apply to the study of what they do such precise analytical experimental methods as will give definite knowledge of at least some of the variables concerned in the determination of why they do it. In short, instead of interpreting what animals do in terms of a crude anthropopsychism why not be objective, and by experimentally modifying and controlling the animal's behavior learn something of the biological processes back of it?

Around 1900 it was pretty unanimously agreed that this was the thing to do, and it was done. For a few years a glib familiarity with "tropisms" and "reflex movements" was as essential to biological respectability as a corresponding acquaintance with "genes" and "crossing-over" is now. Two schools of thought and opinion crystallized, the one led by Loeb and the other by Jennings. They may be characterized, with perhaps the least chance of giving offense to anybody, as respectively the more simply mechanistic and the less simply mechanistic ways of regarding the happenings called life. The two cohorts of followers fought and bled on the battle-fields of "forced movements," "trial and error," and so on, with the utmost nobility and sacrifice of ink.

Quite unfortunately, as it seems to me, this fundamentally important line of research so brilliantly inaugurated, began after a decade or so to languish. Loeb turned off to physical chemistry and Jennings to genetics, and with the generals gone the armies melted away, to ally themselves to what they supposed to be more auspicious, or at least more fashionable movements. The case well illustrates the potency of the sheepish elements in human behavior. For no informed person supposes for a moment that all the problems of animal behavior and comparative psychology have been completely solved. Quite on the contrary the field has just been well opened up. And it is my conviction, based on some personal experience, that there is no other discipline which gives the student such an insight and grasp of fundamentals in the philosophy of biology as does the first-hand study of animal behavior. Every student in training for a career in any field of biology will find it extremely valuable in his future work to have done a piece of careful work in animal behavior under competent direction and guidance.

VII

We come now to the consideration of what, directly and in its numerous ramifications, is the dominant mode in present-day biology. I refer, of course, to experimental evolution. Beginning philosophically as a reaction against the sterility of pure morphology as a method of solving the great problems of organic evolution, it owes its actual origin as a major movement in biological thought to two circumstances, first, the bringing to light of the long-forgotten papers on the mode of inheritance of characters in certain plants by the Austrian monk, Gregor Mendel; and second, to the inauguration of the biometric method in biology by Francis Galton, Karl Pearson, and W. F. R. Weldon. It was plain enough to the writers of the Neo-Darwinian school, as indeed to everybody else who had grasped anything of the meaning of Darwin's work, that the basic factors in organic evolution were variation and heredity. Why not, then, study these factors directly, intensively, experimentally, and quantitatively? There could possibly be but one sensible answer to this question. And because this is so is the reason that genetics and biometry came upon us with such a rush, and have grown and prospered so vigorously.

Bateson, in the address to which I have already referred, tells the story of this change

in viewpoint in the study of evolution very well, and I cannot do better than quote him again:

Discussion of evolution came to an end primarily because it was obvious that no progress was being made. Morphology having been explored in its minutest corners, we turned elsewhere. Variation and heredity the two components of the evolutionary path, were next tried. The geneticist is the successor of the morphologist. We became geneticists in the conviction that there at least must evolutionary wisdom be found. We got on fast. So soon as a critical study of variation was undertaken, evidence came in as to the way in which varieties do actually arise in descent. The unacceptable doctrine of the secular transformation of masses by the accumulation of impalpable changes became not only unlikely but gratuitous. An examination in the field of the interrelations of pairs of well characterized but closely allied "species" next proved, almost wherever such an inquiry could be instituted, that neither could both have been gradually evolved by natural selection from a common intermediate progenitor, nor either from the other by such a process. Scarcely ever where such pairs co-exist in nature, or occupy conterminous areas do we find an intermediate normal population as the theory demands. The ignorance of common facts bearing on this part of the inquiry which prevailed among evolutionists, was, as one looked back, astonishing and inexplicable. It had been decreed that when varieties of a species co-exist in nature, they must be connected by all intergradations, and it was an article of faith of almost equal validity that the intermediate form must be statistically the majority, and the extremes comparatively rare. The plant breeder might declare that he had varieties of Primula or some other plant, lately constituted, uniform in every varietal character breeding strictly true in those respects, or the entomologist might state that a polymorphic species of a beetle or of a moth fell obviously into definite types, but the evolutionary philosopher knew better. To him such statements merely showed that the reporter was a bad observer, and not improbably a destroyer of inconvenient material. Systematists had sound information but no one consulted them on such matters or cared to hear what they might have to say. The evolutionist of the eighties was perfectly certain that species were a figment of the systematist's mind, not worthy of enlightened attention.

Then came the Mendelian clue. We saw the varieties arising. Segregation maintained their identity. The discontinuity of variation was recognized in abundance. Plenty of the Mendelian combinations would in nature pass the scrutiny of even an exacting systematist and be given "specific rank." In the light of such facts the origin of species was no doubt a similar phenomenon.

Now while it is true that genetics has by no means solved the problem of evolution as yet, and probably by itself never can and never should have hoped to, the intensive pursuit of this line of inquiry during the last decade has enormously advanced our knowledge of general biology. In the first place, thanks to the brilliant work of Morgan and his students with *Drosophila,* we have firmly welded the last links in the chain of a definite proof of the causal connection between particular visible details of nuclear structure in the germ cells and particular somatic characters transmitted from parent to offspring in inheritance. The "mechanism of heredity" is no longer a thing to speculate and build broad nebulous hypotheses about. We definitely *know* a good deal about this mechanism and how it works.

In the second place genetics, with cytology as a working partner, as we have already noted, has solved at least in broad outline, the problem of the causation of sex. In the third place, the general results of modern genetic study taken as a whole, and particularly the intensive study of the breeding of animals and plants which the getting of these results has entailed, have made it highly probable, as I think most geneticists, at least, will agree, that natural selection as postulated by Darwin, has had but little if anything *directly* to do with the causation of the evolution of the living things about us. That natural selection is a process always and everywhere going on in nature (except in the case of civilized man, where its operation has been in large degree suspended by virtue of certain attributes of civilization itself) no competent observer of nature can possibly deny. But that it either does or could bring about evolutionary results attributed to it by Darwin seems in the light of our present knowledge, indefinitely more improbable than it did twenty-five years ago. To give all the reasons which exist to support this view would be

wholly impossible with my time limitations. But that these reasons have been convincing to a great number of the most distinguished students of biology in recent years is certain. Because some of them have frankly given expression to their doubts, has led many well-meaning, but wholly uninformed, and somewhat unintelligent, persons to conclude that leading biologists no longer "believe in evolution." Nothing could be more hopelessly wrong than this conclusion. Every biologist who has got beyond a first elementary course in the subject knows that organic evolution is an observed and observable fact of nature, of something like the same obviousness and certainty as the fact that unsupported pieces of matter fall to the earth. I suppose that no one, even a "Fundamentalist," would think of asking a physicist if he "believed in gravitation." It is equally absurd to ask a biologist if he "believes in evolution." But just as one may appropriately discuss today the relative merits of Newton's and Einstein's views as to certain phases of the problems presented by the phenomenon of gravitation, so may he with propriety debate the significance of Darwin's theory of natural selection as a causative agent in the phenomenon of organic evolution.

It must seem to a young man or woman embarking now upon a career in biology that the only thing in the subject of any particular importance is genetics. I wish to point out, with a gravity as becoming as it is difficult to maintain while emitting such a platitude, that this is not true. There is a great deal in biology about which we are abysmally ignorant which partakes neither of chromosomes, nor Mendelism, nor yet of "crossing-over." And, if I mistake not, little light is likely to be shed on these dark places by the just now so brilliantly flaring torches that I have mentioned. The advancement of biology has at least one point in common with another fascinating subject, the adornment of women. Both progress evolutionally by a series of waves of fashion. Just now genetics is the reigning mode in biology. Nothing could be more charming, but it is neither the only nor the final word in charm.

It is apparently hopeless to expect anything like a reasonably balanced development in biological research, and, in consequence, of teaching. And perhaps if we had it we should all be bored. But it can do no harm if we think once in a while about some of the fundamental problems of biology which practically no one is even making an attempt to investigate experimentally, and towards the solution of which we are apparently making little progress. Time will not permit to say all that I should like to on this point, but I feel that I must in some degree indicate that what I have just said about the inadequacy of genetics as at present pursued, is not merely an idle gibe. To this end I shall discuss briefly two matters, adaptation and heredity.

The really difficult problem of evolution is adaptation. The original student of adaptation as a biological problem was Lamarck. It was the problem that lay behind and beneath all of Darwin's work, and he was almost the last investigator who in any systematic way busied himself with the problem. It seems to me that there are only two later students of this problem whose work is of very considerable importance, Hans Driesch and Lawrence J. Henderson. There is an objectively manifest teleology in animate nature. No thoughtful person can fail to be deeply impressed with the ingenuity and beauty with which organisms and their parts are adapted to the attainment of certain ends beneficial to the individual and the race. How came these adaptations about? What is the explanation? In the principle of natural selection Darwin put forward the first and, so far, the only mechanistic explanation of adaptation, though to Hume not Darwin should be given the credit of origination so far as this particular phase of the problem is concerned. It took away, if correct, at one stroke any necessity for the operation of supernatural causes in the explanation of the living world. It was this aspect of Darwin's theory of natural selection which disturbed thoughtful theologians vastly more than the fact of evolution itself, the descent of man from lower animals. For it was and is always possible, even if not plausible, to argue that the Creator chose to work in an evolutionary manner in the building of the world. But a strictly mechanistic

explanation of adaptation, if adequate, destroys completely the very keystone of the arch of any theistic philosophy. Nothing could undermine more completely the prestige of a theistic agency than to prove that it is unnecessary—than to show, in short, that the supposed results of its infinite wisdom and omniscience not only would have occurred, but actually did happen as a result purely of natural, mechanical causes without any external, supernatural intervention.

The question, however, is: did the manifold adaptations which we see in living nature in actual fact arise through the operation of the processes of trial and error and natural selection? A final answer to this question seems to me impossible in the present state of knowledge. In the eighties and nineties the answer would have been, among biologists if not among philosophers, almost unanimously affirmative. Today the case seems much more doubtful. *Formally* it is possible to explain many particular adaptations by natural selection. Some it appears impossible to explain in this way, even formally. What wants intensive investigation is the whole biology, from every conceivable angle, of *particular* adaptations. No more important problem exists. And its difficulty should act as a stimulus rather than a deterrent to its study. To solve it, or indeed to contribute significantly to its solution, will require a different point of view and a different method from that of present-day genetics.

It may seem a little ungracious to suggest, in view of the brilliant results of genetic work which I have already mentioned, and which I yield to no one in admiration of, that the present dominant mode of research in genetics can give us only an incomplete and, philosophically considered, somewhat superficial knowledge of heredity, but I am unable to convince myself that such is not the fact. My views on this point have not changed since I discussed it in detail some seven years ago. I then said[6]:—

Mendelism finds its limitations, just as did the biometric methods in the fact that from the logical standpoint it is essentially a statistical method which studies only the laws of distribution of things given or assumed. It examines only the distribution of hereditary specificities, and not at all, directly, their origin or determination. The former aim cannot be the goal of genetic science. A method which can travel only so far cannot hope to say the last word in the discussion of the problem of heredity. As a mode of research the Mendelian method of analyzing the progeny distributions rather than the ancestral will always be used. It was indeed one of the most brilliant methodological discoveries in the history of science. But it has limitations in the direction of what it can accomplish per se in elucidating the problem of heredity.

It is altogether usual in current discussions of variation and heredity to neglect completely everything which comes between the two end terms of the ontogenetic series, the germ cell on the one hand and the adult soma on the other. But clearly what goes between is a most essential part of heredity itself. It is astonishing how little has been done on these extremely obvious problems.

Two of the four general methods which have been employed in the investigations of the problem of heredity have been seen to be essentially statistical, and two essentially biological. The statistical methods—the biometric and the Mendelian—differ fundamentally only in that the former investigates primarily the ancestry and the latter primarily the progeny. Logically exactly the same distinction was found between the two purely biological methods—the cytological and the embryological. The former studies the ancestry of the germ cell (gametogenesis), the latter the progeny of the germ cell (somatogenesis).

All of these methods are valuable, and each has contributed to our present knowledge of heredity. No one of the methods alone can, however, solve the problem. They all have at least one fundamental limitation in common. This is that they offer no means of directly getting at any definite information regarding the origin, cause, or real nature of that specificity of living material which is the very foundation of the phenomenon of heredity. The distribution of hereditary specificities, their putative morphological "bearers," and many other things about them have been studied more or less exhaustively. The things themselves have been speculated about, but not investigated to any but the slightest extent.

6 Pearl, R. Modes of Research in Genetics. New York (Macmillan), 1915.

VIII

In bringing to a close this brief and inadequate review of the major trends of biology I want to say a few words about a purely practical movement which is rapidly gaining force and seems likely shortly to have a pronounced effect upon the development of the whole subject, including its theoretical aspects, and particularly its teaching. I refer to the rapidly growing recognition of the fact that all of the activities of all living things, including man, are properly a part of biology in a greater or less degree. The practical importance of this lies in its corollary that the biologist may and probably does have something important to contribute towards the solution of the most various sorts of human problems, agricultural, medical, social, economic, and so on. During the last quarter of a century it has been increasingly forced upon the attention of university teachers of biology that students of sociology, of philosophy, of medicine, of economics, and of many other subjects, who had no intention to become professional biologists, not only wanted to, but needed to know something about biology. At first covertly resisted, this need is now frankly being recognized and in some degrees met by the reorganization of courses, and departures of varying degree from the traditional method of teaching this subject. This is, I think, entirely healthy and desirable. There is going along with this broadening of the viewpoint of biological teaching a welcome broadening of the opportunities for a useful and profitable career in biology. There are already many kinds of applied biology attracting young men and women. And quite beyond the range of these somewhat narrow specialties, we are witnessing such phenomena as the employment of research workers in general biology by a great corporation manufacturing electrical appliances, to mention but a single instance.

To one who embarked upon a biological career twenty-five years ago, solely because he was seduced by the charm of the subject, and who in yielding renounced, against the advice of family and friends, the supposedly certain and considerable rewards which would come if he continued, as he had tentatively started, on a career in which he might finally become a teacher of Greek, the opportunities for the biologist of the present day seem somehow humorously magnificent.

If in what I have said I have succeeded in any degree in indicating the intellectual justification of Dr. John A. Lichty's splendid gift to Mount Union College for the endowment of its flourishing department of biology, my principal object will have been achieved. Under the able leadership of Professor M. J. Scott we may confidently expect the work of the department to go forward in close touch with each new and promising field of endeavor which biology presents. I can not allow myself to close without expressing, as a biologist, my deep admiration and profound respect for the breadth of vision and deep philosophical insight which is implied in the endowment by a worker of the field of medicine of a chair of general biology. The Milton J. Lichty Chair of Biology is another enduring demonstration of the fact that the most enchanting of all the sciences has really come into its own.

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EARTH-CURRENT OBSERVATIONS[1]

The Department of Terrestrial Magnetism of the Carnegie Institution of Washington is planning to install earth-current lines for systematic observations at its magnetic observatories. During this year such lines are being installed at the Watheroo Magnetic Observatory, about 120 miles north of Perth, Western Australia, and some time later similar installations will be made at the Huancayo Magnetic Observatory, about 125 miles east of Lima, Peru; both of these magnetic observatories are conducted under the auspices of the Department of Terrestrial Magnetism. Various initial investigations concerning best methods of earth-current

[1] Presented before the Philosophical Society of Washington, February 25, 1922. The full paper is published in the March-June, 1922, issue of *Terrestrial Magnetism and Atmospheric Electricity*, pp. 1-30.

measurements are at present in progress at the Department's laboratory in Washington.

In order to take advantage of the previous experience gained in earth-current work, and to ascertain the direction in which further study is desirable, the writer undertook a discussion of the available data, especially of the 11-year series, 1910-1920, obtained at the Observatorio del Ebro, Tortosa, Spain. For the first time comparisons could be made between the phenomena of terrestrial magnetism, earth currents, and atmospheric electricity, as dependent upon extensive observations at the *same* station. Accordingly, it has been possible not only to confirm and extend certain results previously reached by others, but also to draw important new conclusions.

It is hoped that the present investigation, which had to be confined to a discussion of the observational data on magnetically-calm, or on electrically-calm days, may be supplemented later by a discussion of earth-current data on disturbed days.

The chief conclusions may be stated as follows:

(*a*) The resultant horizontal earth-currents, as observed at the Ebro Observatory, flow, on the average for the year, in the direction from about 29° west of North to 29° east of South, or, approximately, in the direction from the Magnetic North Pole towards south-southeast. The average value, for the magnetically-calm days during 1914-1918, of the potential gradient of the component of the current flowing from true North to South was 0.20 volt per kilometer, and that of the component towards geographic East was 0.11 volt per kilometer, or about one half of the north-south component. The resultant horizontal potential-gradient was 0.23 volt per kilometer, which during electric or magnetic storms may reach a value 0.8 to 1.0 volt per kilometer.

(*b*) The annual variations of the earth-current potential-gradients and of the components of the Earth's magnetism, as observed at the Ebro Observatory, may be related to one another as cause and effect only to a very minor extent; both sets of variations may have to be referred, more or less, to common causes. The range of the annual variation of the north-south electric component is about 2.5 times that of the west-east component.

(*c*) The diurnal variation of earth currents as observed at the Ebro Observatory along lines somewhat over one kilometer long is remarkably similar to that observed at Berlin along telegraph lines, 120 and 262 kilometers in length, from 1884-1887. In both cases the diurnal variations for the component of the current along the meridian is considerably more pronounced (2-3 times) than that along the parallel of latitude. The diurnal variation in the north component of the earth's magnetism is not such as to correspond to the direct magnetic effect of the diurnal variation of the west-east component of the earth currents. A similar conclusion had to be reached with regard to the east component of the earth's magnetism and the north-south component of the earth currents. The general conclusion was that the north-south earth-current might be the result of electro-magnetic induction, caused by the fluctuation during the day of the west-east component of the earth's magnetism. *If it be recalled that all analyses of the diurnal variation field of the earth's magnetism have shown that the magnetic diurnal variation is in part to be ascribed to electric currents circulating in the regions overhead and in part to currents circulating within the earth's crust, exact agreements between magnetic variations and earth-current variations are not to be expected. It further remains to point out that until we have some knowledge of the actual course or distribution of the earth currents in the earth's crust and as to how the conductivity of the crust may vary with temperature and other meteorological causes during the day and at the actual place of observation, attempts to find a quantitative relationship between terrestrial-magnetic and earth-electric effects may be futile.*

(*d*) The horizontal vector-diagrams both for the magnetic and earth-electric components vary during the sun-spot cycle in about the same proportion. The earth-current vector-

diagram is symmetrical about a line approximately in the direction of the Magnetic North Pole.

(*e*) The extreme diurnal range of the Ebro earth currents reaches its highest values near the equinoctial months, and lowest near the solstitial months. Earth currents, atmospheric electricity, the Aurora Borealis, and the earth's magnetic disturbances, all show similar annual variations in the ranges of their fluctuations.

(*f*) The potential gradients of earth currents and of atmospheric electricity apparently vary during the sun-spot cycle, the former decreasing in the direction of normal flow of current, and the latter increasing with increased sun-spot activity. The diurnal ranges of the potential gradients of earth currents, as well as of atmospheric electricity, just as is the case for the diurnal variation of terrestrial magnetism, increase with increased sun-spot activity.

(*g*) There is evidence of a similar six-hour wave in atmospheric electricity, earth currents and terrestrial magnetism.

The analyses referred to in (*c*) are chiefly those by Schuster, Fritsche, Chapman, Walker, and Miss van Vleuten, the method of investigation employed by them being that first suggested by Gauss, which is based on the well-known Amperian rules of deflection of a magnetic needle by an electric current. The general result reached by these investigators, as stated in (*c*), has been accepted by every modern magnetician; it post-dates the investigations by Airy and Weinstein quoted by Dr. Sanford in his recent article[2]. In this connection it may be pointed out that the conclusions drawn by Dr. Sanford do not depend upon simultaneous earth-current and magnetic data at the *same* station, as was the case in my investigations.

As stated above, my present conclusions apply only to possible relations between the *diurnal variation* phenomena of earth currents and of the earth's magnetism. It does not appear that definitive conclusions can be safely reached until we have at the *same* station unquestioned coincident magnetic and electric data, and until we can furthermore consider in our comparisons only that portion of the magnetic diurnal variation caused by systems of forces below the earth's surface.

A fresh examination is also being made regarding the relations between earth currents and severe disturbances of the earth's magnetism, such as occur during the so-called magnetic storms. There are some indications which may support the views recently advanced by Satyendra Ray[3], though I am not prepared just now to make a definite statement.

With the view of giving renewed stimulus to systematic earth-current investigations, a special committee, "to consider and report on best methods and instruments," was formed at the Rome meeting of the International Section of Terrestrial Magnetism and Electricity last May. The chairman of the committee is Sir Arthur Schuster, and the secretary, Dr. S. J. Mauchly, of the Department of Terrestrial Magnetism.

LOUIS A. BAUER

DEPARTMENT OF TERRESTRIAL MAGNETISM,
CARNEGIE INSTITUTION OF WASHINGTON

[2] Earth currents and magnetic variations, SCIENCE, October 27, 1922, p. 466.

[3] Ray, S., "Ueber parallele Störungen von parallelen erdmagnetischen und erdelektrischen Elementen," Zs. Physik, Berlin, v. 7, 1921 (201-205).

COLLABORATORS IN THE STANDARDIZATION OF BIOLOGICAL STAINS

FROM time to time reports from the Committee on Standardization of Biological Stains have appeared, dealing with the investigations in progress. Many congratulations have been received by the chairman of the committee on the results accomplished; but as these accomplishments would have been impossible but for the very hearty collaboration of a long list of investigators, credit for the work should be given where it belongs by publishing the following list of committee members and collaborators:

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The work so far accomplished by these collaborators includes: an extensive study of American methylen blues, fuchsins, gentian violets, and eosins for bacteriological purposes; a study of eosin, methylene blue, hæmatoxylin, orange G and safranin for various histological purposes; while work is in progress at present on a number of other stains, including methylen green, Bordeaux red, brilliant green, brilliant cresyl blue, cresylecht violet, pyronin, and acid fuchsin. The results accomplished are so promising that there is reason to believe that the most commonly used stains can be regarded as standardized before the following winter is over. It will then be possible to work out some method of certification of stains which come up to the standards.

None of this work would have been possible but for the cooperation of such a large number of investigators, who have responded to every call for assistance in a most gratifying way. It was not anticipated at the start that such a large number would be found to take part willingly in an investigation of this sort. The work, of course, has been entirely voluntary. The committee would like, whenever reporting on any stain, to give due credit to all of these collaborators but as such a course is impractical the best plan seems to take the present occasion to make their names public and express appreciation for their assistance.

H. J. CONN,
Chairman
COMMITTEE ON STANDARDIZATION OF STAINS,
NATIONAL RESEARCH COUNCIL

SCIENTIFIC EVENTS

THE RAMSAY MEMORIAL

THE unveiling of the tablet in Westminster Abbey in memory of Sir William Ramsay, to which reference has been made in SCIENCE, was the last act in connection with the memorial, a history of which is summarized in the London *Times*. In 1917 an appeal was issued for £100,000 by a committee, under the presidency of Mr. Asquith, and under the chairmanship of the late Lord Reay. At a subsequent date, the Prince of Wales became patron of the fund. The sum collected in cash is £57,645.

In addition, the fund has been augmented

by a number of research fellowships instituted by various dominion and foreign governments, of which the capitalized value is estimated at about £60,000, so that the total sum raised in response to the appeal may be regarded as being nearly £120,000. This sum is believed to be the largest ever raised in any country as a memorial to a man of science.

The sum collected in cash includes subscriptions from Great Britain and Ireland, America, Australia, Canada, Chile, China, Denmark, France, Greece, Holland, India, Italy, Japan, New Zealand, Norway, Straits Settlements, Switzerland and Portugal.

The following governments have instituted fellowships of the value of £300 a year: Canada, Greece, Italy, Norway, Sweden, Denmark, Spain, Holland, while the Japanese government has instituted a fellowship of the value of 4,320 yen (approximately £463). French and Swiss fellowships have been instituted, part of the cash contribution in those countries being used for the purpose.

These fellowships are intended to lead to an orientation of many of the most promising young scientists of the world to England. Chemists from Norway, Sweden, Denmark, Holland, Switzerland, Japan and the United States are already at work in England. The Italian and Greek fellowships are at present vacant, and the Spanish felowship has not yet been filled, although it has been provided. Fellows are studying in London, and at Oxford and Cambridge, at the Imperial College of Science and Technology, Glasgow, and at Liverpool. A number of British fellows are also at work.

The Ramsay Committee has carried out a number of the objects which were set out in the original appeal. A sum of £25,000 has been laid on one side for the purpose of a laboratory of chemical engineering, to be established at University College, London, where Sir William Ramsay held his professorship for twenty-six years. This building has not yet been erected, though arrangements are now in progress.

A sum of £14,000 was handed over to a body of trustees, consisting of Sir George Beilby, Sir Hugh Bell, Lord Crowe, Mr. H. A. L. Fisher, Sir Donald MacAlister, Dr. J. C. Irvine and Sir Robert Hadfield, for the purpose of founding Ramsay Memorial Fellowships in Chemical Science for British students. Each fellowship is of the value of £300. In addition, a sum of £6,000 in respect of Glasgow subscriptions was handed over to the same trustees to provide a fellowship of £300 a year for a Glasgow candidate.

A medal has been struck from a design of the French sculptor, M. L. Bottée. A sum of £210 has been paid to University College, London, for the institution of a Ramsay Medal from M. Bottée's design, to be awarded annually to the most distinguished student of chemistry at University College.

There remains a small balance of the Ramsay Fund, after providing for the cost of the memorial tablet, the disposal of which has not yet been definitely settled.

THE ZEITSCHRIFT FÜR PRAKTISCHE GEOLOGIE

DR. PHILIP S. SMITH, acting director of the U. S. Geological Survey, permits us to print the following letter from Dr. Franz Beyschlag, president of the Geologischen Landesanstalt, Berlin:

On account of the sad financial conditions in our country it is probably known to you that the question of the existence of the *Zeitschrift für praktische Geologie,* published by me and my colleague Krusch, is at stake. Cost of printing and postage have risen so high that we shall be compelled to discontinue the *Zeitschrift* in a short time, unless help comes. From the request of your librarian to the publisher of the *Zeitschrift,* Wilhelm Knapp in Halle, I gather that there is a lively demand in America for this *Zeitschrift.* From that I conclude with right that there is an interest in the existence of our publication and that it is not unlikely that some subscribers can be obtained. Therefore I would be especially thankful to you if you would endeavor to secure in the interested circles there a considerable number of subscriptions. The publisher could send the numbers regularly through the American Institute in Berlin so that there would be no postage. In this way you would render the

Zeitschrift an extraordinary service and it might perhaps be possible to keep it alive. For our common strivings in the field of practical geology it would be calamitous if this Zeitschrift, after so long existence, was now compelled to go under through financial difficulties. It is a good medium for scientific publication so that authors also would suffer through the passing of the publication. For your efforts in the interest of this matter, I pledge my highest thanks.

SIGMA XI AT THE UNIVERSITY OF IDAHO

THE thirty-eighth chapter of Sigma Xi, to be known as the Idaho Chapter, was installed at the University of Idaho on June 5. Fifteen active members, who were elected to the society while connected with other educational institutions, composed the petitioning group.

The installation exercises were conducted by Dr. Henry B. Ward and Dr. Edward Ellery, president and secretary of the national society. The charge to the chapter was delivered by Dean Ellery and the symposium was conducted by Dr. Ward. Eighteen science men who were formerly associated with the University of Idaho Sigma Xi Club were invited to attend the installation ceremonies. Several active members of the Washington State College faculty were also in attendance.

The following officers were elected: *President,* Dr. J. E. Wodsedalek; *vice-president,* Dr. M. F. Angell; *secretary,* Dr. Henry Schmitz; *treasurer,* Professor C. E. Behre.

A formal banquet was held in the evening at Lindley Hall, University Campus. Among one hundred guests of the chapter were Dr. Ward, Dean Ellery, Dr. A. H. Upham, president of the university; Dr. E. A. Bryan, commissioner of education of Idaho; Miss Ethel Redfield, state superintendent of public instruction; Dr. Melander, of Washington State College; members of the Idaho Board of Regents, the associate members of the former Idaho Sigma Xi Club, and the deans and heads of the arts and letters divisions of the university. Dr. J. E. Wodsedalek acted as toastmaster and toasts were responded to by Dr. Ward, Dean Ellery, President Upham and Commissioner Bryan.

The installation took place during commencement week. Dean Ellery delivered the baccalaureate sermon and Dr. Ward gave the commencement address. Dean Ellery's subject was "The hills of human help," while that of Dr. Ward was "The struggle of man with wild life in North America."

THE ASSOCIATION OF AMERICAN GEOGRAPHERS

THE annual meeting of the association, in connection with the Geological Society of America, will be held at the University of Michigan, Ann Arbor, by invitation of the regents of the university, on Wednesday, Thursday and Friday, December 27, 28 and 29, beginning on Wednesday at 1:30 P.M.

The president's address will be given at the opening of the session Thursday afternoon and will be followed by a series of invited papers. Later in the afternoon, by special invitation, the association will visit an exhibit of rare maps belonging to Mr. William S. Clements, a regent of the university.

Sessions will be held in the natural science building where exhibition and smoking rooms are available. Members desiring to exhibit papers, maps, etc., should inform Professor C. A. Sauer in advance.

The arrangements include:

Round table conference (open to members and invited guests), Wednesday evening, December 27.

Smoker tendered by the regents of the university, Thursday evening, December 28.

Luncheon, as guests of the university, Friday noon, December 29.

A joint meeting for physiography papers with the Geological Society of America.

The secretary will be glad to receive the names of non-members to whom the preliminary program should be sent. All interested in geography, or any of its allied subjects, are welcomed at the program sessions of the association.

HARLAN H. BARROWS,
President

RICHARD E. DODGE,
Secretary

STORRS, CONNECTICUT,
NOVEMBER 7, 1922

THE ECOLOGICAL SOCIETY OF AMERICA

The Ecological Society of America will hold its eighth annual meeting at the Massachusetts Institute of Technology on Wednesday, Thursday and Friday, December 27, 28 and 29, 1922. In addition to the general sessions, joint meetings will be held with the American Society of Zoologists and the Botanical Society of America.

Titles and abstracts of papers to be placed on the program should be in the hands of the secretary by December 1, in order to be printed for distribution before the meeting. Persons not members of the society may present papers on the recommendation of a member.

A dinner to which all persons interested in the society and its activities are invited will be held at the Athens Café, 694 Washington Street, on Wednesday evening. The hotel headquarters will be at the Parker House, Tremont Street, where rooms will be available at rates from $2.50 up. Reservations should be made at an early date.

A. O. Weese,
Secretary

James Millikin University,
Decatur, Illinois

THE AMERICAN SOCIETY OF NATURALISTS

The fortieth annual meeting of the American Society of Naturalists will be held in Boston, Massachusetts, on Friday, December 29, in the buildings of the Massachusetts Institute of Technology.

The headquarters of the society will be at the Hotel Somerset, which is also the headquarters of the American Association for the Advancement of Science. Rates at the Somerset are as follows: One in a room, with bath, $4 to $6; two in a room, with bath, $5 to 7. Members are advised to make reservations early. Information concerning other accommodations will doubtless be available for the final announcement of the society in December.

The morning session will be devoted to a celebration of the birth of Gregor Mendel and of Francis Galton in 1822. For this program the following speakers have been secured: E. M. East, T. H. Morgan, J. Arthur Harris and George H. Shull.

A symposium on Geographical Distribution has been arranged in conjunction with the American Society of Zoologists. For this program, papers by Glover M. Allen, Thomas Barbour, E. R. Dunn, C. H. Eigenmann, P. P. Calvert and C. T. Brues have been secured. It may be possible to announce others later.

A memorial to the late Professor Sedgwick, in the form of a lecture by Professor E. B. Wilson, is scheduled for Friday afternoon, closely following the Naturalists' symposium on geographical distribution.

Following instructions given by the Naturalists last year, the officers of the society participated, during the year, in conferences looking toward federation of the biological societies of America. Notices concerning these conferences have several times appeared in Science. The final result of these meetings is a proposed constitution of such a federation which was printed in Science for September 29. It is expected that reprints of this article in Science will be distributed to members of the society along with the final announcement in December. The question of accepting membership in the federation will be presented at one of the business meetings of the society.

The annual dinner, with the address of the president, Professor W. M. Wheeler, will be given on Friday evening at the Hotel Somerset.

Blank forms for the nomination of candidates for membership in the society may be obtained from the secretary. Attention is called to the rule that nominations must be in the hands of the executive committee at least a year before being acted upon. Accordingly, nominations to be voted upon in 1923 must reach the secretary before the close of the meeting of 1922.

A. Franklin Shull,
Secretary

University of Michigan,
Ann Arbor, Michigan

SCIENTIFIC NOTES AND NEWS

At the dinner of the Natiional Academy of Sciences, held in connection with the New York meeting on November 15, the Draper Gold Medal was presented to Professor Henry Norris Russell, of Princeton University. Dr. C. G.

Abbot, director of the Smithsonian Astrophysical Observatory and secretary of the academy, a former recipient of the medal, made the presentation address and Professor Russell replied.

Friends of Professor Chandler presented in 1910 to Columbia University a fund which constitutes the Charles Frederick Chandler Foundation. The income is used to provide a lecture by an eminent chemist and for a medal to be presented to the lecturer. Previous lecturers on this foundation have been Dr. L. H. Baekeland, W. F. Hillebrand, W. R. Whitney, F. Gowland Hopkins and Edgar F. Smith. The lecturer this winter will be Dr. Robert E. Swain, professor and head of the department of chemistry, Stanford University, whose subject will be "Atmospheric pollution by industrial wastes." The lecture will be in Havemeyer Hall, Columbia University, on January 9, 1923, at 8:15 P.M.

M. Arthus, professor of physiology at the University of Louvain, has been elected correspondent of the Royal Academy of Medicine of Belgium. Dr. Depage has been chosen the recipient of the quinquennial prize for the best work published in the field of medical science for the period 1916-1920.

Professor de Castro, dean of the Medical School of Rio de Janeiro and delegate from Brazil to the League of Nations, was given an ovation recently at Paris, in the presence of the ambassador from Brazil and many of the professors and students of the university. De Castro delivered an address on monoglandular and pluriglandular dystrophies.

The following physical chemists have been named as editors of the *Journal of Physical Chemistry:* By the Chemical Society of London, Professors T. M. Lowry, J. W. McBain and James C. Philip; by the Faraday Society, London, Professor F. G. Donnan; by the American Chemical Society, Dr. A. L. Day, Professor G. A. Hulett, Dr. Irving Langmuir and Professor W. Lash Miller. Through the efforts last summer of Dr. Charles L. Parsons, secretary of the American Chemical Society, this publication, edited by Dr. Wilder D. Bancroft, of Cornell University, hitherto a strictly American journal, was brought under the auspices of the two leading British societies and the American group.

Mr. Kenneth M. Gould, formerly associate editor of the *American Journal of Public Health,* has severed his connection with the American Public Health Association and the American Social Hygiene Association, to become editor of the publications for the Rockefeller Foundation. The *Journal* will be edited in future by Dr. Henry F. Vaughan, commissioner of health of Detroit, Michigan, assisted by an editorial board composed of Dr. M. P. Ravenel, of the University of Missouri, and Mr. A. W. Hedrich, secretary of the association.

Mr. Willis H. Rich, of California, has been appointed assistant in charge of the division of scientific inquiry of the Bureau of Fisheries. Mr. Rich has been connected with the bureau's investigation work on the Pacific Coast salmon since 1913, having been closely associated with Dr. Charles H. Gilbert, of Stanford University.

C. Robert Moulton is leaving the University of Missouri, department of agricultural chemistry, with which he has been associated for the past fifteen years, to become head of the Bureau of Nutrition for the Institute of American Meat Packers in Chicago.

Mr. Paul Croll, formerly research chemist of the New Jersey Zinc Company, has been engaged by the Patton-Pitcairn Division of the Pittsburgh Plate Glass Company.

The *Journal* of the American Medical Association states that Professor Ramón y Cajal was not able to be present at the recent unveiling of the portrait statue at the medical school of Zaragoza, but a letter from him was read in his name, saying that he doubted whether he would have had courage to witness the unveiling of his statue even if his health had allowed it. He said he feared the statue would ask, "What have you done to deserve this honor? Are you not ashamed to be so distinguished when no memorial has been erected to . . ." and he named several Spaniards who had won international recognition among the scientists of their day. He added that the car of Spanish

civilization has been running along too long on merely the two golden wheels of art and literature. It needs two more wheels to keep abreast with the rest of the world, a wheel of science and a wheel of industry.

Edwin F. Hopkins has resigned as plant pathologist of the University of Missouri to accept a position as plant physiologist with the Marble Laboratory, Inc., of Canton, Pa. He will be engaged in a study of problems related to cold storage.

Leave of absence has been granted by the Corporation of Yale University to Dr. Lafayette B. Mendel, Sterling professor of physiological chemisry, to enable him to deliver a course of lectures on the Hitchcock Foundation at the University of California in the late spring of the present university year. It is the intention of Professor Mendel to leave New Haven after the dedication of the Sterling Chemistry Laboratory in April, to join the faculty of the University of California for the intersession, which continues from May 14 to June 23, 1923. Professor Mendel has chosen for his subject "New aspects of the physiology of nutrition."

Dr. Charles H. Gilbert, of Stanford University, California, who during the past summer has made an extensive investigation of the salmon fisheries in the Alaska Peninsula Fisheries Reservation, created in February, 1922, was in Washington from October 18 to 26 conferring with officers of the Bureau of Fisheries regarding conditions that he had found in the reservation, outlining future work to be taken up there and discussing the regulations necessary for the calendar year 1923. Dr. Gilbert visited Seattle on November 16 and 17 for the purpose of conferring with people operating in the Alaska Peninsula Reservation and discussing permits that will be issued for the operations that will be allowed in the reservation the coming year.

A Danish scientific mission, under the leadership of Professor Olufson, accompanied by the French savant, Professor Bourcart, of the Sorbonne, left Paris early in the present month on a six months' expedition in the northern Sahara, where it will cover a distance of some 3,000 miles. The members of the mission include the botanist, Dr. Gram, and the geologists, Drs. Storgaard and Kayser. The party, which will start from Tunis, intends to make a detailed study of the Shat-el-Jerid. From Nefta it will proceed to Tuggurt, and thence to Wargla, in the Algerian Sahara. Next it will go to Insalah, and endeavor to explore the Hoggar Mountains.

Chas. R. Fettke, associate professor of geology and mineralogy at the Carnegie Institute of Technology, Pittsburgh, Pa., has completed an investigation of the oil resources of the coals and carbonaceous shales of Pennsylvania for the State Bureau of Topographic and Geological Survey.

Dr. Charles P. Berkey, professor of geology at Columbia University, has returned from China where he was with the Third Asiatic Expedition of the American Museum of Natural History.

Dr. Edgar F. Smith, former provost of the University of Pennsylvania and president of the American Chemical Society, gave a lecture at the University of Pennsylvania on November 3 on Joseph Priestley, under the auspices of the Priestley Club.

A course of eight lectures on "Secretion and Internal Secretion" was given by Professor Swale Vincent, M.D., D.Sc., professor of physiology in the University of London, at Middlesex Hospital Medical School, during November.

Dr. Joseph S. Ames, professor of physics at the Johns Hopkins University, director, Office of Aeronautical Intelligence of the National Advisory Committee for Aeronautics, spoke on November 23 before the Franklin Institute of Philadelphia, on "Recent aeronautic investigations and the airplane industry."

Dr. August Krogh, professor of zoophysiology in the University of Copenhagen, lectured at the University of Pennsylvania on November 14 and 15 on "Nervous and hormonel control of capillary contractility" and on "The exchange of substances through the capillary wall." Dr. Krogh addressed a special meeting of the Entomological Society of Washington

on November 8 on the subject of insect respiration.

Dr. Hugh Potter Baker, executive secretary of the American Paper and Pulp Association, formerly dean of the New York State College of Forestry, lectured on "Forests and forestry in New England" before the Middletown (Conn.) Scientific Association on November 14.

Professor Elias Judah Durand, chairman of the department of botany in the University of Minnesota, died at his home in St. Paul on October 29, of cancer. He was born in Canandaigua, N. Y., in 1870 and after graduating from Cornell University in 1893 became a fellow, assistant and instructor in botany at the university. In 1910 he went to the University of Missouri as assistant professor of botany, being made associate the next year. In 1918 he was called to the University of Minnesota as professor of botany. He was the author of important contributions to mycology.

UNIVERSITY AND EDUCATIONAL NOTES

The annual report of the treasurer of Yale University for the year ending June 30 records an unusually large number of gifts, made to meet the conditions of the $3,000,000 subscribed to general endowment by Mrs. Stephen V. Harkness. As a result of these contributions and the establishment of six new professorship funds in memory of John W. Sterling, '64, of almost $250,000 each, the total of Yale's endowment funds is shown to be $32,662,011.95, an increase of $6,985,001.25 in the last year. Gifts for building and other non-permanent funds received in the same period aggregated $1,651,290.68, while gifts to income amounted to $740,642.24. Included in the latter were contributions of $185,000 from the General Education Board and $30,000 from the Commonwealth Fund to enable the Yale School of Medicine to provide funds for the reconstruction of two wards in the New Haven Hospital, and to build laboratories in that institution, with which the school is affiliated; $70,000 more from the Commonwealth Fund towards the expenses of the department of surgery; and $286,664 received through 9,493 contributors to the Alumni Fund, the principal of which was also increased by $147,060.41.

The West Indian Agricultural College, which was formally opened by Sir Samuel Wilson, governor of Trinidad and Tobago, on October 16, has begun its session in a temporary building erected at St. Augustine. Eighteen students have been enrolled, including three post-graduates, and this is regarded as a promising start for a new institution of the kind. Tenders have been invited for the construction of the permanent college building, of which Major H. C. Corlette, is the architect.

Dr. Charles Wesley Flint was inaugurated as chancellor of Syracuse University on November 17.

Dr. Edmund C. Sanford, president emeritus of Clark College and at present head of the department of psychology, has been appointed acting president of Clark University in the absence of President Wallace W. Atwood. The trustees have granted to President Atwood a four months leave of absence for travel in Europe. President Atwood expects to visit the leading educational institutions of Europe, and will observe teaching methods in western Europe.

Dr. Stanhope Bayne-Jones, associate professor of bacteriology at Johns Hopkins University, has been appointed professor of bacteriology at the University of Rochester.

Dr. Howard de Forest, formerly of the Indianapolis Normal School science department, and of the botany department of the University of Kansas, has been appointed assistant professor of botany at the University of Southern California in Los Angeles.

Miss Elizabeth Evans Lord, psychologist for the Chicago Juvenile Court, has been appointed clinical and research assistant in the Yale University Psycho-Clinic.

Dr. H. Stanley Allen, of the University of Edinburgh, has been called to the chair of natural philosophy in the United College of St. Andrews University, which became vacant at the end of the last academical year by the retirement of Professor Arthur Butler.

DISCUSSION AND CORRESPONDENCE

RELATIVITY

To the Editor of Science: Like many others, I commonly read whatever, from books to mere notes, by Dr. Edwin E. Slosson, comes to my notice. Generally I am well pleased, but an exception has just occurred. I very much dislike that pleasantly written article on Relativity in the *Scientific Monthly* for November, 1922. I dislike it because, giving the words used the only meanings recognized by layman and scientist alike, save a few specialists, several of the assertions are sheer nonsense. Certainly no system of equations, however clever, can prove to one of common sense, the existence of a real fourth dimension; that time and space are not wholly independent; that just because we and the Martians may be unable to synchronize our clocks there is no 'now'; that time is "curved"; that a phenomenon may be seen before it happens; that the mere inclusion of gravitation in a more comprehensive expression eliminates it from nature; and so forth, and so on, through a long list of absurdities—absurd, that is, if their customary meanings be given to the words used.

Such expressions catch the attention, because they seem to declare the truth of amazing paradoxes, but they are, after all, mighty poor paradoxes, for their whole secret is nothing but the assigning of strange meanings to familiar words; a sort of cryptic writing. Naturally, all such "crazy" expressions, crazy so long as unexplained, inevitably breed contempt for science and the scientist.

Let us, then, in popularizing the thoughts of specialists, first understand clearly just what those thoughts are, and then put them in the words and circumlocutions of the other fellow. The real relativist is not playing hob with our understanding of nature, however different his descriptions of certain phenomena may seem; but if the language of his average popularizer is to be taken literally, and no hint, as a rule, is given of any other meaning, more topsy-turvy indeed than the Land of Alice is this finite, limitless universe that simultaneously will be, was, and is.

W. J. Humphreys

TINGITIDÆ OR TINGIDÆ

In connection with this subject there are some other points which I think should be mentioned. The Ionic genitive Τίγγιοζ and the Attic genitive -εωζ show without a doubt that the word Τίγγιζ is an ι-stem. In Latin it would be an i-stem, Tingi, and the genitive Tingis.

That there is a Latin word Tinge of which the stem is Tingit does not concern us for Fabricius did not use it. He could easily have done so had he wished. While these words have the same root they have different stems. The International Rules instruct us to add -idæ to the stem of the name of the type genus. They do not expect us to worry about other words based on the same root. Fabricius was a Greek purist and he based his name on the word Τίγγιζ, -ιοζ (Ionic, -εωζ (Attic). In writing this word in Latin he did so correctly using Tingis in the genitive. The stem of the name of the type genus is, therefore, Tingi. The family name correctly should be Tingiidæ.

It is unfortunate that Westwood omitted one i in writing the family name but before the days of the International Commission this was sometimes done. We often write Mantidæ for example based on Mantis, genitive -ιοζ (Ionic), -εωζ (Attic). If we follow the International Rules we must insert the other i and write Tingiidæ. And most of us agree that the rules should be followed.

A. C. Baker

Bureau of Entomology

A CHEMICAL SPELLING MATCH

In Science for October 20, Dr. L. O. Howard comments in rather facetious vein upon a chemical spelling match described in the number for September 29. He mentions his struggles with chemical names during the twenty years he was permanent secretary of the A. A. A. S. and rather approvingly drags in a quotation from Forel, who seemed to think that no true scientist uses long words. Dr. Howard is more specific and applies this to chemistry. He arouses not the resentment but the sympathy of the chemist because of the suspicion that he is envious of a body of knowledge (call it science

for short) that has such a precisely descriptive and stable system of nomenclature as chemistry.

The chemist, if diligent, can make at least one new compound every day or so and in his spare moments give it a name. Often it is easier than deciding what to call a new baby. The name he gives will generally stick, because only on rare occasions does some other chemist come along and show that the harness got twisted when the radicals were hitched up. Then all that is needed is to rearrange the component parts of the name or to substitute "ortho" for "para" or "meta."

The name tells what the substance is. Doubt arises when a short and easy name is applied. For the chemist a good name is rather to be chosen than great wealth of description, because it is self-contained. The naturalist must have detailed descriptions, preferably with plates, and is happiest when he can make comparison with "type specimens."

In his spare moments the botanist or zoologist digs around in old books and journals with the hope of resurrecting an old name for some familiar plant or animal. This is called stabilizing the nomenclature. It is done because such and such a congress decided that the race for supremacy and final adoption shall be won, not by a name that has come swiftly down the years and is known by all, but by one that stayed at scratch, hidden in some dusty volume.

Shuffling the cards for a new deal is another delightful diversion. For such names as X........ a........ (Smith) Jones *comb. nov.* special honors are awarded, particularly to Jones. The pity of it is that somebody else may come along and soon the specimen becomes Y........ b........ (Brown) White *comb. noviss.* In this way the nomenclature becomes fixed.

What is queer about a chemical spelling match? To name a compound for which the formula is given, or to do the reverse, is good training for the memory. Can one imagine a botanical or an entomological spelling match? Could "aster" or "grasshopper" be drawn in recognizable detail by the contestants? The optimistic chemist will concede that the respective drawings could with some confidence be labelled "flower" or "bug," but could an expert name the species? Yet the pitifully unscientific chemist who uses long words to cloak his ignorance can at once tell the correct names of two such closely related species as H_2SO_3 and H_2SO_4.

C. E. Waters

Washington, D. C.
October 27, 1922

MUSCINA PASCUORUM MEIGEN IN NEW ENGLAND

This European fly has made its appearance in considerable numbers this year in Massachusetts and Connecticut. The first specimen was collected in Connecticut, August 6, and it is still (November 14) quite common in the vicinity of Boston. The muscid is about three times the size of the house fly, bluish black, with a whitish, pruinose covering. A detailed account is in preparation and any information as to its further distribution will be greatly appreciated.

Charles W. Johnson

Boston Society of Natural History

SCIENTIFIC BOOKS

The Minds and Manners of Wild Animals. By William T. Hornaday, Sc.D., A.M. New York: Charles Scribner's Sons, 1922. Pp. x + 328.

If every man devoted to his affairs, and to the affairs of his city and state, the same measure of intelligence and honest industry that every warm-blooded wild animal devotes to its affairs, the people of this world would abound in good health, prosperity, peace and happiness.

To assume that every wild beast and bird is a sacred creature, peacefully dwelling in an earthly paradise, is a mistake. They have their wisdom and their folly, their joys and their sorrows, their trials and tribulations.

As the alleged lord of creation, it is man's duty to know the wild animals truly as they are, in order to enjoy them to the utmost, to utilize them sensibly and fairly, and to give them a square deal.

With these reflections, the dean of scientific directors of American zoological parks presents his volume on the minds and manners of wild animals. And with the following picture —reproduced here only in part—the curtain falls:

On one side of the heights above the River of

Life stand the men of this little world—the fully developed, the underdone, and the unbaked, in one struggling, seething mass. On the other side, and on a level but one step lower down, stands the vanguard of the long procession of ''Lower'' Animals, led by the chimpanzee, the orang and the gorilla. The natural bridge that *almost* spans the chasm lacks only the keystone of the arch. . . . .

The great Apes have traveled up the River of Life on the opposite side from Man, but they are only one lap behind him. Let us not deceive ourselves about that. Remember that truth is inexorable in its demands to be heard.

Into this book Dr. Hornaday has put much of his philosophy of life as well as the choicest of his observations on the behavior of wild animals in nature and in captivity. The moral purpose which impelled the writer to expression is the defense of dumb creatures. Our author takes special pains to humble man by dwelling on his shortcomings. The reader is told that, though endowed richly with mind and gifts of expression and therefore capable of noble achievement in service and self-development, man at his worst is the most bestial of animals and more brutal than the so-called brutes.

"The minds and manners of wild animals" will disappoint not a few scientific students of animal behavior because it is not an exact systematic and analytic description of animal experience and action. It will delight almost everyone else by its directness, sincerity and naturalness. For the tens of readers who may get next to nothing from the book because of the "experimentalist bias"—to which the reviewer must plead somewhat guilty—there will be thousands who gain useful knowledge, insight and a more intelligent appreciation of wild animals.

The book should be taken, in the opinion of the reviewer, as a notable contribution to natural history, not as a scientific treatise on comparative psychology. It contains a wealth of amusing, interesting, thrilling and enlightening incidents and personal observations, a somewhat biographical assemblage of reflections and conclusions and a unique thought-provoking collection of brief characterizations of animal intelligence and temperament. Such is the contribution to animal behavior and rights which Dr. Hornaday has made from his almost unexampled wealth of experience as zoologist, hunter and scientific director of zoological gardens. The information presented should be of very considerable practical value to all who have to do with wild animals.

It would be a profitless task to discuss in SCIENCE the scientific grounds of dissatisfaction with a book which is primarily an account of personal experiences with wild animals. Conspicuous among them are terminology, definition, canons of judgment, inferences and generalizations. Such matters every scientific reader will note, but will he nevertheless be able, as the layman almost certainly will, to enter into and profit by the author's lifetime of intimate contact with wild animals? Let us hope so.

More to the point than a recital of the content of this volume is the injunction, "Read it and thus enter into the author's knowledge, sympathetic appreciation and insights." Truth is great. The ways of observing it are as varied as human intellect and temperament. It were a pity to lose the value of the naturalistic in our praiseworthy attempts to exalt the experimental study of animal behavior and experience.

ROBERT M. YERKES

SPECIAL ARTICLES

PROOF OF THE POWER OF THE WHEAT PLANT TO FIX ATMOSPHERIC NITROGEN

IN a series of wheat cultures in solutions, we have recently proved conclusively that wheat plants, even in only six weeks of growth, can fix large quantities of nitrogen from the air. They possess this power whether nitrogen is supplied to the roots or not.

Seventeen years ago, Jamieson[1] made the startling announcement, based on experiments, that all green plants possess the power of fixing atmospheric nitrogen. He supplemented this announcement by another to the effect that

[1] *Report of Agr. Res. Assn.*, Aberdeen, 1905, *et seq.*

special organs exist on the young leaves of plants whose function it is to fix the air nitrogen and he called these organs "albumen generators." This supplement to Jamieson's first announcement and the somewhat loose statement of his proffered evidence on the nitrogen fixation, coupled with the indelible impression of Lawes and Gilbert's and Boussignault's experiments and conclusions, caused the scientific world as a whole to scout or ignore Jamieson's evidence and the earlier contentions of Ville and a few others to the same effect. In 1911, Mameli and Pollacci[2] published a statement of experimental results which were not subject to the criticisms pertinent in Jamieson's case and which proved conclusively that a variety of green plants possess the power of fixing atmospheric nitrogen. Later statements by them[3] only confirmed their earlier assertions. They did not accept Jamieson's supplementary statement relative to the mechanism of the fixation in question.

Even the world of science is so conservative as not to have caused a general acceptance of the contentions of Jamieson and Mameli and Pollacci, despite the fact that Moore and Webster[4] and Moore, Webster and Whitley,[5] as well as Wann,[6] have more recently furnished ample confirmatory evidence with fresh water and marine algæ as material. In order to furnish further evidence for securing an acceptance of this new view of nitrogen fixation which is directly opposed to the old established view and to obtain data for the wheat plant which has not been studied in that regard, the writers have recently carried out an experiment resulting as indicated in the general conclusion introducing this brief note. Wheat plants were grown in "Shive's best" solution of an osmotic pressure of 1.3 atmospheres. These solutions were so constituted as to have approximately the same concentration throughout, regardless of whether or not nitrogen was present. The containers for the solution were quart Mason fruit jars and the usual technique was employed. Five wheat seedlings per jar, and six jars of each kind of solution were employed, thus testing thirty plants with each solution. In the case of the solution containing no nitrogen, twelve jars were employed, six of them being kept in the greenhouse until seeds were formed. All the other plants were grown for a period of six weeks only. It is impossible now to go into the many interesting features of this and other experiments which we are conducting on the important subject of nitrogen fixation. Suffice it to say that in a number of series of wheat plants grown without nitrogen and with varying quantities of nitrate, definite evidence was adduced that all the wheat plants fix nitrogen from the air. Even excluding the nitrogen content of the culture solutions at the end of the experiment, because of some irregular data in the analyses, there is a gain of nitrogen from the air which varies in different series from 13 to 21 per cent. of the total amount of nitrogen found in the plant. With the nitrogen in the solutions taken into account, these values will be much larger.

A series of experiments with barley is now being completed, and promises to yield similar results to those obtained with wheat. Legumes and other plants will now be studied, and many other features of the subject investigated. There can be no question now, however, that the teaching of all our books, and nearly all our teachers on the subject to-day are erroneous and must be changed completely to accord with the facts presented by us, and by the other investigators whom we have cited above. As Moore and Webster have put it, authority has too long held sway over logic and experimental fact. It is high time to let those considerations rule. A full, theoretical and historical discussion of this problem will be given in the detailed account of our experiments.

C. B Lipman
J. K. Taylor

2 *Atti dell'Instituto Botainco della R. Universita de Paria*, Vol. 13, p. 351.

3 *Ibid.*, Vol. 14, p. 159, and Vol. 16, p. 197.

4 *Proc. Roy. Soc. Lond.*, Series B, Vol. 91, p. 201 (1920).

5 *Ibid.*, Vol. 92, p. 51 (1921).

6 *Amer. Jour. Bot.*, Vol. 8, pp. 1-29, January, 1921.

THE AMERICAN CHEMICAL SOCIETY

DIVISION OF AGRICULTURAL AND FOOD CHEMISTRY

C. S. Brinton, *secretary*

T. J. Bryan, *chairman*

JOINT SYMPOSIUM WITH DIVISION OF BIOLOGICAL CHEMISTRY

Subject: Edible Fats and Oils. Part A—Manufacturing and Technical

David Wesson, *chairman*

Manufacture of edible fats and their compounds: L. M. TOLMAN.

Refining losses in the manufacture of edible oils: B. H. THURMAN. Each step through the refining process will be discussed relative to various vegetable oils, cotton, peanut, soya bean and cocoanut. The action of refining materials in removing undesirable products and impurities is the largest source of shrinkage on most oils. Methods for determining the percentage of impurities, such as lecitro-proteins and coloring matter, will be given, both from laboratory and factory determinations. There are emulsions formed and broken, which are described in detail, giving some experiences of handling them in the factory. One that is not yet handled successfully and causes loss should be interesting to the colloidal chemist. Another step in the process illustrates selective absorption by Fuller's earth and carbon black. Losses due to volatility and solubility are accounted for with averages for different vegetable oils.

Corn oil—its preparation and uses: A. F. SIEVERS. Corn oil is produced as a by-product in the hominy and cornstarch industries. From eighty to one hundred million pounds are produced annually, of which about 70 per cent. is refined for food purposes. Corn oil is classed as a semi-drying oil but has poor drying qualities and therefore does not enter largely into the manufacture of paints. It is used in the manufacture of soap and in making its greatest progress for practically serving the same purposes as cottonseed and peanut oils. Its physical and chemical properties are similar to cottonseed and soya bean oils. The oil prepared from dry process germs is generally lighter in color and contains less free acid than that made from wet process germs.

Edible fats in the baking industry: CHARLES A. GLABAU. This paper is based on the data obtained in our laboratory which to us is quite interesting especially where the homogenizer has entered in. The paper bears the following subtitles: (1) Introductory; (2) defining the various kinds of bakery products in which edible fats are used; (3) how the fats are introduced and incorporated; (4) why fats are added to bakery products; (5) the results obtained by adding graduated quantities of fat to bread doughs (stereopticon plates); (6) tracing the fat through the dough mass with coal tar derivatives (stereopticon plates); (7) introducing a new method of incorporating fat and mixing the dough; (8) the results obtained by homogenizing fats used in the bakery; (9) the distribution of emulsions through the dough mass; (10) determining the carbon dioxide diffused through doughs containing prepared emulsions and doughs in which the fats are incorporated in the general manner; (11) conclusion.

The action of shortening in the light of the newer theories of surface phenomena: WASHINGTON PLATT and R. S. FLEMING. The following definition of shortening and shortness is used: "Shortening is any fat or fixed oil used as an ingredient in baked products. That material has the greatest shortening power which, when baked in a dough under standard conditions, gives to the product a minimum breaking strength and a minimum crushing strength." A cookie is seen to be essentially a mass of gluten and starch, soaked in a concentrated sugar solution. Shortening is the only material in dough not soluble in water or wetted by it. Shortening brings about its effects by extending throughout this dough or cake in layers which separate the particles of the dough or cake from one another and prevent the formation of a continuous solid mass. When care is taken to prevent change of the specimen on mounting, the fat may be seen microscopically in the dough and cake, extending in films around the starch grains. An investigation was made to determine the cause of the difference between the shortening power of the common fats. Viscosity, surface tension *vs.* air and melting point considered alone are seen to be of minor importance. Plasticity is seen to be a more important factor. The work of Langmuir and of Harkins on phenomena at liquid interfaces is correlated with the differences in shortening power. The close connection between the action of shortenings and of lubricants is emphasized.

Certain physical and chemical requirements of fats in the evaporated milk industry: HARPER F. ZOLLER. The evaporation of milk in a vacuum pan at the temperature and pressure under factory operation necessitates the consideration of factors in connection with the constitution and physical make-up of fats which are uncommon in

all other industries in which fats are used. The fat should have an iodine absorption number below 30. It must contain a minimum of fatty acid esters which, when hydrolized, will yield fatty acids possessing unusual flavors or odors (*e. g.*, arachidic, theobromic, erucic, ricinoleic, etc.). Its "ethyl ester value" should be quite high, preferably, in the purified natural fat, above 12. The content of stearin and pulmitin should not be high enough to raise the melting point above 50° C. It should be a fat which is readily purified and should not therefore contain substance such as phytosterol, sitosterol, alkyl amines, etc. Happily enough, cocoanut oil and palm nut oil which are widely used in the margarine industry because of their availability and physical properties come closer to these requirements than do any of the commercial fats save butter fat. Inasmuch as the margarine industry used cocoanut oil it was but natural that the compound milk industry should choose it. From the standpoint of condensation in the vacuum pan in the presence of the milk, a good grade of cocoanut oil works more admirably than does butter fat itself. The same may be said of palm nut oil. The high iodine number of some butter fats, 28-42, renders it subject to slight rancidification (hydrolysis) in the vacuum pan and subsequent sterilization. Partially hydrogenated cotton oil may be used providing its iodine number is kept about 30, so that its melting point will not interfere with the pan process.

The analytical detection of rancidity: ROBERT H. KERR. The analytical tests used for the recognition of rancidity, the chemical and physical differences between rancid and sweet fats, and will give some consideration to the mechanism of rancidity and the changes involved in its development.

Rancidity and a method for its detection: H. C. BASHIOUM and R. J. NOBLE. Rancidity of two types, "A" and "B"—"A due to volatile fatty acids, "B" due to volatile fatty acids and aldehydes. Rancidity "B" detected and comparatively estimated by means of Schiff's Reagent (pararosaniline acetate dissolved in dilute sulfurous acid solution). A 0.5-1.0 per cent. solution of the oil in kerosene or preferably benzene is shaken with an equal volume of the reagent in a separatory funnel, continuously or intermittently for 30 minutes. If the oil be rancid, a violet to blue coloration will appear immediately or within a few minutes in the benzene or kerosene layer. The color developed is proportional, within limits, to the degree of rancidity. The test is very delicate and especially suitable for the detection of "B" in cereal products containing small amounts of oil.

The oil, fat and wax laboratory, Bureau of Chemistry, Department of Agriculture, and its relation to the vegetable oil and fat industry: GEORGE S. JAMIESON and WALTER F. BAUGHMAN. An account of the vegetable oil and fat investigations conducted by this laboratory, discussed under three heads: Olive oil and its substitutes, supply of fats and oils during the war and fundamental investigations. It is almost impossible to get adulterated olive oil past the barriers at our ports. The small amount of adulterated oil on the market is sophisticated in this country by small firms. During the war our imports of fats and oils exceeded our exports. The first complete survey of the fat and oil industry was made. It was not possible to increase production of cottonseed oil, but production and importation of peanut and soya bean oils were greatly increased. Many new possible sources of oil were investigated. The chemical composition of some of the vegetable oils have been determined. A representative number of authentic samples of cottonseed and peanut oils have been analyzed to establish the limits of variations in the chemical and physical characteristics of these two oils. A new method has been developed for determining the amount of neutral oil in crude oils. Work is in progress on the isolation and identification of all constituents of cottonseed oil and their effect on refining.

Colorimetry as applied to the vegetable oil industry: DAVID WESSON. Cottonseed oil has always been sold on color and various means have been devised from time to time for reading and recording color. Modern conditions have called for more accurate instruments than those used in the past, using Levibond Tintometer glasses. The Eastman Kodak colorimeter furnishes an ideal instrument for measuring and recording the color of samples where colorimetric measurements only are desired. Where it is necessary to analyze a color as in research work, the new Keuffel and Esser color analyzer is to be preferred.

A brief note on the examination of the fat from Theobroma grandiflora: W. C. TABOR.

A rapid quantitative method for the determination of arachidic-lignoceric acid mixture in peanut oil: ARTHUR W. THOMAS and CHAI-LAU YU.

The chemical composition of sunflower seed oil: GEORGE S. JAMIESON and WALTER F. BAUGHMAN. Sunflower seed oil is used in various foreign countries as a food oil and in making butter substitutes, soaps, varnishes and enamels. Several million pounds of the seed are produced annually

in the United States for poultry feed and the production could be greatly increased. The whole seed contains 27 per cent. to 30 per cent. oil and the kernels which constitute about 53 per cent. of the seed contain approximately 53 per cent. oil. It is a drying oil. S. G. 25/25 is 0.9193; refractive index 20°, 1.4736; iodine number (Hanus), 130.8; saponification number, 188.0; unsaponifiable matter, 1.2 per cent.; saturated acids, 7.1 per cent.; unsaturated acids, 86.6 per cent.; iodine number of unsaturated acids, 147.9. The oil consists of glycerides of the following acids: oleic, 33.4 per cent; linolic, 57.5 per cent.; palmitic, 3.5 per cent.; stearic, 2.9 per cent.; arachidic, 0.6 per cent.; lignoceric, 0.4 per cent.

The chemical composition of soya bean oil: Walter F. Baughman and George S. Jamieson. The oil was pressed from mammoth yellow variety of soya beans by an expeller. Specific gravity, 25°/25°, .9203; refractive index 20°, 1.4736; iodine number (Hanus), 128.0; saponification value, 189.5; unsaponifiable matter, 0.6 per cent.; saturated acids, 11.5 per cent.; unsaturated acids, 83.5 per cent.; iodine number of unsaturated acids, 148.7. Bromine addition derivatives of unsaturated acids were made and analyzed. The methyl esters of saturated acids were fractionally distilled under diminished pressure and fractions analyzed. Oil was found to consist of glycerides of following acids: linolenic, 2.3 per cent.; linolic, 51.5 per cent.; oleic, 33.4 per cent.; palmitic, 6.8 per cent.; stearic, 4.4 per cent.; arachidic, 0.7 per cent.; lignoceric, 0.1 per cent.

On the use of n-butyl alcohol in the determination of the titer test of fats and oils: H. A. Schuette and J. H. Draize. A study was made of the effect of substituting n-butyl alcohol for glycerol or ethyl alcohol as a saponifying medium in the official methods for determining the solidifying point of the mixed fatty acids, or titer test, of fats and oils. Inasmuch as the melting point, iodine absorption number and titer test of the mixed fatty acids of a series of fats and oils were found to be substantially the same when sodium n-butoxide, glycerol potash or a hydroalcoholic solution of sodium hydroxide were used as saponifying agents, it is concluded that the former may be substituted for the others without loss of accuracy. A complete and more rapid saponification, without scorching of the resulting soap, is possible.

The fat soluble vitamin: H. C. Sherman. This paper constitutes a review of present knowledge of the fat-soluble vitamin with special reference to recent advances and practical applications to some food problems. While the fat-soluble vitamin has been studied mainly by means of experiments upon rats, it is now known to have very important functions in the nutrition of animals generally, including men. It is needed by adults as well as during growth. A food supply containing only enough of the fat-soluble vitamin for growth will not support full vigor. When the food furnishes too little of the fat-soluble vitamin the body is weakened and becomes more sensitive to infection. On the other hand the body is able to store this vitamin in cases in which the food furnishes more than is required for current needs. The vitamin should be reckoned with as an important factor in food values. The supplementary relationship between foods of high fuel value and those of high vitamin value is discussed.

Color vs. vitamin content of fatty foods: Leroy S. Palmer.

Commercial vitamin preparations: Washington Platt.

Thoroughness of digestion of different kinds of fats and oils: C. F. Langworthy.

Studies of the vitamin potency of cod liver oils. II. The effect of season on the vitamin potency of cod liver oil—spring oil. Arthur D. Holmes. The present paper is one of a series reporting experiments undertaken to determine the vitamin "A" potency of cod liver oils obtained at different seasons of the year. To obtain oils of known origin the author personally obtained oils of cod livers from cod fish and rendered the oils under laboratory conditions. Attention is being given to other factors which vary during the year, such as physical condition, sexual activity and diet of fish. Tests with early spring oil from emaciated fish show that .00202 grams of oil daily is fully adequate for the vitamin "A" growth requirements of albino rats.

The influence of light on the synthesis of vitamin A in sprouting white and yellow corn: J. S. Hughes and W. R. Horlacker. A sample of yellow corn having a high vitamin A content and one of white corn having a low vitamin A content were sprouted both in the light and dark. The vitamin A content of the sprouts not including the grain was tested by the usual feeding test with rats. The sprouts from both the white and yellow corn grown in the light had a high vitamin A content. The sprouts from neither the white or yellow corn grown in the dark contained much of this vitamin. The results indicate that the vitamin A content of the seed has very little influence on the vitamin A content of the sprout, but that the sunlight is an important factor in the synthesis of vitamin A.

Suggestions in technic vitamin work: Edward

F. KOHMAN. We hear much about the destruction of vitamins by the action of heat and oxidation. Vitamin A is said to be especially sensitive to oxidation and vitamin C to both heat and oxidation. But with the exception of a very few instances, the experiments from which such conclusions are derived do not justify an assumption as to whether the destruction noted was really the result of heat or oxidation. No reference has been found in any experiments relating to the effect of heat and oxidation in which the oxygen content of the product or of the cooking water has been taken into account. Practically all fruits contain more or less atmospheric oxygen both in solution and mechanically trapped. To eliminate this a high vacuum is not sufficient unless the container is jarred by rather sharp blows. More important is the oxygen held in solution by the water used for cooking. This can not be removed with less than five minutes boiling, nor is a vacuum effective unless the container is jarred. For temperatures less than boiling, hours are required, and during this time the dissolved oxygen would be more available for oxidation of the vitamin than the oxygen of the air which is in contact with the surface. Air-free water dissolves air very readily and therefore must be kept out of contact with air until used.

The chick as an experimental animal in vitamin studies. II—With respect to the fat-soluble vitamins: A. D. EMMETT and GAIL PEACOCK. Continuing the study of comparing the chick with the rat and pigeon the findings relative to the fat-soluble vitamins indicate in the case of the White Leghorn breed that they are fairly suitable for test purposes. In marked contrast with the pigeon, the fat-soluble vitamin requirements of the chicken are very much greater. The most prominent symptoms are weak legs, partial paralysis, enlarged knee joints, dropping wings, weak eyes, accompanied by xerophthalmia which stimulates roup, diminished pigmentation of comb, bill, legs and feet, labored breathing, poor oxidation and loss in weight. In other words, the chick (male or female) needs both the antirachitic and the antiophthalmic fat-soluble vitamins, while the pigeon appears to need relatively little of either. A lack in these requirements is apparently more markedly evidenced in chicks three to four weeks old than in those that are six to nine weeks old. In fact, the onset of the symptoms are so rapid in the younger animals that it is very difficult to bring about a correction of the ailments before death ensues. For practical test purposes our data, which includes some 600 birds, suggest that chicks about seven weeks old are the most suitable. Compared with the rat, the chick has its limitations as a test animal. It occupies more space, consumes more food, has a greater range of variation in rate of growth, being more difficult to handle and bring through the early and critical period of growth.

Milk and ice cream as fatty foods: JEROME ALEXANDER. Since milk contains 88 per cent. of water, the legal 3 per cent. fat means 25 per cent. of the total solids, so that milk is a fatty food. When the casein coagulates it mechanically entraps the fat, thus forming a greasy curd which is hard to digest. Cows' milk has a low protective ratio, *i. e.*, the ratio of casein to lactalbumin, and therefore readily forms greasy curds unsuitable for the human stomach. Increasing the protective ratio by adding any colloidal protector (gelatin, eggs, etc.) prevents this difficulty, and is of especial importance in ice cream, where the fat content is much higher than in milk. The effect of colloidal protection in artificial milks and cheese is still to be worked out.

A new method for the determination of vanillin: H. C. BASHIOUM and FRED Y. HERRON. This method depends upon the properties of the aldehyde group rather than those of the phenolic group as is usually the case. In practice, the solution containing vanillin is saturated with hydrogen sulfide in the presence of hydrochloric acid, whereupon a white precipitate of thiovanillin is produced. The precipitate is filtered off, washed with water and then dissolved in dilute sodium hydroxide solution in which it easily dissolves, producing a yellow color, the intensity of which is proportional, within limits, to the concentration of thiovanillin.

The soda equivalent of sour milk used in baking: MARY P. WILSON and H. A. WEBB. Baking soda and sour milk can not be titrated together with any known indicator. A method of preparing alizarine test paper of approximately N/20 strength (one No. 3 quinine capsule of soda in ¼ standard cupful of water), drying and "spotting" with mixtures of the N/20 soda, measured in drops, and ¼ teaspoon of the sour milk under examination, was worked out. When the spot shows no color change, the number of spots made permit calculation of the equivalent amounts of baking soda or baking powder per cup of milk of any degree of acidity.

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